

October 7, 1954

Dear Josh,

Thanks for your thoughtful letter and the solicitude it expresses for me. Several of my good friends have feared that the Moewus affair has already reflected badly on my reputation -- or that it may. I cannot concern myself about this since, first, it makes no difference (except that it could adversely affect my present students), and second, I believe I have been right in my objectives if not always in my actions, and, third, my major concern is, as it should be, the Chlamydomonas story and not what other people think of my relation to it.

I my search for information about events at Woods Hole I have spoken to Ruth Sager and Bernie Davis, and telephoned Sonneborn. I have had three long and dreadful sessions with Moewus and his wife, and many with a number of less directly involved parties. My only intention was to secure facts from the source so that, first, I could inform M. of where he stood, and second, I could help keep rumors from deviating as widely from the truth as they have already.\* I feel there is nothing to gain but uncertainty and unhappiness from an inquisition into motives, or sometimes even facts, in an historical situation. The possibility of a judgement will exist only after an honest attempt to reproduce Moewus' experiments under his own conditions, and this has never been tried to my knowledge, even by Hartman's people.

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\* For example, even your own unusually accurate account of the cro<sup>-</sup> mutant story deviates from that of Moewus and Ruth which agree with one another. You write: "This he did (make a new preparation), and so successfully demonstrated a non-motile preparation, except that they were noticed to be dead!" Apparently many of the cells were flagellated and motile when taken directly from the agar slant, more became so with time, in water. Ruth's suspicion is that M. added the iodine to prevent the development of flagella by the residue of unflagellated cells. (M., by the way, claims that the flagellated character of the cro<sup>-</sup> mutant, and of all of his strains at the time, was an unexpected function of the lamp he was using -- a different one which he had used for the class demonstration had blown out -- and that they are not flagellated now.)

I guess I made a mistake in letting M. go to Woods Hole in my absence. I underestimated the suboptimal conditions in the laboratory and the subtlety of interested parties. I simply thought the MBL would be a fine place for M. to show his stuff to students who would have to do his experiments and to interested colleagues. I earnestly hoped some would take his material and try to reproduce some experiments, or even follow him to Columbia. After all, no one, not even Ruth, had tried to do one thing despite M's presence at Columbia since January. This was a disappointment in view of my original objective in inviting M. to Columbia.

I certainly did not intend to become a Chlamydomonas worker, but, after consultation with Sonneborn, it seemed that getting M. in the U.S. would allow Ch. workers and prospective Ch. workers (e.g. from among Sonneborn's recent Ph.D.'s) an opportunity to work with Ch. with first-hand instructions from M. M. wrote that this was just what he wanted - an opportunity to demonstrate his experiments. I appreciated at the time that the outcome of attempts to reproduce this work could fail as well as succeed, but I felt so strongly the need for attempts at repetition that I was prepared to face either outcome.

It is living through this period of trial that is so hard! As long as M stays in my laboratory I must keep confidence in him. I am glad to note that you had the intelligence to see this. He must believe that I am not against him, but rather a friend who could help him show he is right. I must continue to be as patient and calm and firm as I can. A rash judgement in either direction on my part could be disastrous. I will not now enter his laboratory and demand to see the cro<sup>-</sup> mutant or anything else. But I will succeed in having him present me with the opportunity to do his experiments with his material under his instruction. And this will not require a year.

Please don't concern yourself about my being duped. M. appreciates that I must be able to repeat these experiments while he is away from Columbia. I don't remember whether I discussed with you at Ann Arbor my abortive attempts to do some things with Ch. last June while M. was at Woods Hole. I have made ~~some~~ observations on his material as well as on this complex man at work. It is too soon to pass a judgement and consequently I have avoided publicizing my feelings. When I can say to my own satisfaction that I can or cannot

reproduce his experiments, I will let the world know.

Please thank Esther for the copy of the translation of the two papers. I had seen the first in MS last spring (it is out in the July issue of Z.f.N.) and had heard about the results ~~much~~ earlier from Tübingener friends. The second MS, which is even harder for me to assess because of the absence of details, I did not know about.

Glad to learn that our letter to Pres. Hatcher turned the tide in Markert's favor.

My best to both of you and to the Cavallis.

Sincerely,

A handwritten signature in cursive script that reads "Francis". The signature is written in black ink and is positioned below the word "Sincerely,".